

Biogeosciences Discuss., referee comment RC4
<https://doi.org/10.5194/bg-2021-99-RC4>, 2021
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Comment on bg-2021-99

Anonymous Referee #3

Referee comment on "The application of dendrometers to alpine dwarf shrubs – a case study to investigate stem growth responses to environmental conditions" by Svenja Dobbert et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-99-RC4>, 2021

General comments

In this paper, alpine dwarf shrubs are studied with the help of point dendrometers and micrometeorological measurements. The work definitely has a certain novelty, as the application of this set of methods to an alpine shrub is new, as well as the attempt to study dendrometer data on an hourly resolution. The work and its results fit into the current attempts to understand growth processes at high temporal resolution and thus get closer to the underlying physiological mechanisms (see also Zweifel et al., 2021).

I support the publication of the paper in Biogeosciences Discussions, however the current version of the manuscript suffers from some shortcomings that need to be addressed first.

1)

The title is far too bold and needs to be changed, as the paper does not present a new mechanistic understanding of ecophysiological patterns (what is even meant by this term?). Novel aspects of the work are: the application of automated point dendrometers to shrubs and the approach of converting stem radius data into high temporal resolution growth data. It is a case study with limited general relevance, but nicely demonstrates the potential of the methods used.

A title such as the following proposal would better fit the content of the paper:

The application of point dendrometers to alpine dwarf shrubs - a case study to investigate

stem growth responses to environmental conditions.

2)

The authors mix three currently available methods to separate dendrometer data into the components of irreversible growth and reversible water-related stem tissue dynamics.

First, there is the approach of Deslauriers et al. 2007 and Van der Maaten et al. 2016, which treat each individual day independently of the historical evolution of stem radius changes. Therefore, this approach records any absolute increase in stem size over 24 hours as growth.

This is in contrast to the zero-growth approach of Zweifel et al. 2016, where accumulated shrinkage must be replenished over longer periods of time (days and weeks) before any additional stem increase is considered growth. Essentially, the zero-growth approach assumes that no growth is possible during periods of stem shrinkage. All daily increments add up to an annual increment represented by the total annual stem size change measured by the dendrometer. In the case of the Deslauriers et al. approach, daily increments sum to more than the increment measured by the dendrometer over one year. The reason for this is that stem size increases during periods of stem shrinkage are counted as growth.

The third approach is the Gompertz growth function, which takes up the commonly assumed growth form over a season. The Gompertz function assumes constant and uniform growth throughout the growing season, which is clearly not true, as shown in Zweifel et al. 2021 and also in this paper (Fig. 3).

Further, the Gompertz function is not a reliable way to find the onset of growth because the Gompertz function is fitted to the original dendrometer curve and thus neglects that the stem is first rehydrated before growth begins. Furthermore, the nature of the Gompertz function implies a slow growth start at the beginning of the season, which obviously does not fit the growth pattern, as can be well seen in Fig. 3b. This approach leads to too early growth starts in spring and might also be the reason why the authors set a threshold for initial growth at 20% of annual ring growth.

Anyway, all approaches may have advantages and disadvantages, the problem with this work is that there is no clear line that tells me as a reader which analyses and which figures are based on which approach. Also, I don't see how the Deslauriers approach and

the zero growth approach are compatible in the same study.

3)

The statement (L435ff) that winter shrinkage of woody stems has never been reported is false. See e.g. Winget & Kozlowski, 1964; Zweifel & Häslner, 2000; Sevanto et al., 2012.

4)

Legends of figures and tables must be completed with all abbreviations that occur. In addition, the data basis (model or measured data, temporal resolution) should be stated in each case. A legend must be readable on its own.

5)

The analysis in Fig. 4 states that the authors are able to perfectly predict annual growth from radiation with 100% accuracy! This must be wrong!!!

6)

Several recent studies show the importance of VPD for growth (Novick et al., 2016; Grossiord et al., 2018; Peters et al., 2021; Zweifel et al., 2021). It would add weight to this study if VPD were included. If I understand the measurement setup correctly, the authors have this data.

7)

It might be a matter of style, but why are so many results shown already in M&M?

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